LITTLE BLUE BOOK NO. 693
Edited by E. Haldeman-Julius

New Experiments in Animal Psychology

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CONTENTS

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CHAPTER I

INTRODUCTION

Isolated observations of animal activities are, of course, recorded in the works of a vast number of ancient and mediaeval writers, but the science now known as *animal psychology*, *comparative psychology*, or *animal behavior*, is a comparatively recent growth. The student who wishes to orient himself in the literature of the subject need go no farther back than George Romanes, who brought out a book called *Animal Intelligence* in 1883. This work presents a great collection of vastly entertaining anecdotes—country tales of stags, hares, dogs, parrots and the like—with a running fire of comment by Romanes, but very few records of scientifically controlled experiments. *Animal Intelligence* was the best book obtainable for many years, and is amusing and suggestive and well worth reading even today, but it must not be taken too seriously. Many of the animal stories in Charles Darwin's *Descent of Man*, which appeared about 1874, should also be taken *cum grano salis*. These men were interested in the defense of the theory of organic evolution, and in the heat of battle there was a tendency to humanize animals so as to bridge the psychic chasm which was supposed to separate man from the brutes. Lloyd Morgan's

Animal Life and Intelligence, published in 1891, and a book called *Mind in Evolution*, by L. T. Hobhouse, also belong to the anecdotal, pre-experimental period of animal psychology. In the same category must be placed the works of many later field-naturalists, such as John Burroughs, Wesley Mills, Ernest Thompson-Seton and scores of others.

The general procedure adopted by these writers in obtaining information about animals is known as the method of anecdote, and, while it is certainly valuable in calling attention to certain specific problems, it has not proved particularly efficacious in finding out just what animals actually do, or recording their specific responses to specific situations. These anecdotes are so often related by people who know too little, or infer too much, or are too anxious to tell a good story. One of the best illustrations of the perils of ignorance in this field was pointed out by Mr. and Mrs. Peckham, justly celebrated for their excellent work on the habits of spiders and solitary wasps. The Peckhams quote Wilhelm Wundt's famous story of the spider that propped the door open, which tale is best related in the great introspectionist's own words.

"When I was a boy," said Wundt, "I had made myself a fly-trap like a pigeon cote. The flies were attracted by scattering sugar and caught as soon as they had entered the cage. Behind the trap was a second box separated from it by a sliding door, which could be opened or shut at pleasure. In this I had put a large garden spider. Cage and box were

provided with glass windows on the top, so that I could quite well observe anything that was going on inside. . . . When some flies had been caught, and the slide was drawn out, the spider of course, rushed upon her prey and devoured them. . . . This went on for some time. The spider was sometimes let into the cage, sometimes confined to her own box. But one day I made a notable discovery. During an absence the slide had been accidentally left open for some little while. When I came to shut it, I found that there was an unusual resistance. As I looked more closely, I found that the spider had drawn a large number of thick threads directly under the lifted door, and that these were preventing my closing it. . . . I imagine that as the days went by there had been formed in the mind of the spider a determinate association on the one hand between free entry into the cage and the pleasurable feeling attending satisfaction of the nutritive impulse, and on the other between the closed slide and the unpleasant feeling of hunger and inhibited impulse. Now in her free life the spider had always employed her web in the service of the nutritive impulse. Associations had therefore grown up between the definite positions of her web and definite peculiarities of the objects to which it was attached, as well as changes which it produced in the positions of certain of these objects,—leaves, small twigs, etc. The impression of the falling slide, that is, called up by association the idea of other objects similarly moved which had been held in their places by threads properly

spun; and finally there were connected with this association the other two of pleasure and raising, unpleasantness and closing, of the door."

Thus Wundt, in presenting his pet spider with feelings, ideas, associations and so on, infers altogether too much, for there is no real evidence for any of these things. He makes another ridiculous blunder by reason of his ignorance of the ordinary responses of the spider tribe, because—but hear the Peckhams: "Had Wundt been familiar with the habits of spiders, he would have known that whenever they are confined they walk around and around the cage, leaving behind them lines of web. Of course many lines passed under his little sliding door, and when he came to close it there was a slight resistance. These are the facts. His inference that there was even the remotest intention on the part of his prisoner to hinder the movement of the door is entirely gratuitous. Even the simpler mental states that are supposed to have passed through the mind of the spider were the products of Wundt's own imagination." It would be difficult to find a better example of the inherent worthlessness of the method of anecdote than this story of Wundt's. The retailer of anecdotes, as Washburn well points out, is seldom satisfied merely to tell what the animal did and let it go at that—he must needs discourse at length about what the beast *thought* and *felt*, which are obviously matters of which we can know nothing whatever.

Even if the raconteur did stick to the un-

embellished facts, his tales would give us no representative pictures of animal life, because it is only the exceptional, the bizarre, which finds its way into anecdote. As Thorndike puts it: "Dogs get lost hundreds of times and no one ever notices it or sends an account to a scientific magazine. But let one find his way from Brooklyn to Yonkers and the fact immediately becomes a circulating anecdote. Thousands of cats on thousands of occasions sit helplessly yowling, and no one takes thought of it or writes to his friend the professor; but let one cat claw at the knob of a door, supposedly as a signal to be let out, and straightway this cat becomes the representative of the cat-mind in all the books."

The animal psychologists of the present day rely upon a very different procedure, which has been called the method of experiment. This method has brought comparative psychology out of the wilderness and into the laboratory, and placed it on the same scientific level as physiology and experimental zoology. The writings of these moderns are perhaps less sensational and exciting than the marvelous tales of the pioneers in the field, but they are the result of conscientious scientific work under rigidly controlled experimental conditions, and a handful of such papers is worth more to science than tons of the pleasant fiction of Romanes and his followers.

Of course, animal psychology did not become a science over night, and the experimental method is not absolutely new even in this field; one can find quite a number of experimental

studies which date back to the early eighties. Even Romanes, as long ago as 1881, carried out a few valuable experiments with jelly-fish and sea-urchins, and some strictly scientific laboratory work was recorded by Sir John Lubbock, whose *Ants, Bees and Wasps* appeared in 1883. Vitus Graber, a German entomologist, brought out a very good book on vision in animals. W. Preyer, later famous as a worker in child psychology, was experimenting with starfish in 1886, while the late Jacques Loeb, for forty years the principal champion of mechanistic biology, began to publish about 1888. Max Verworn, the physiologist, carried out the first important experimental study of the protozoa in 1889. And there were a few others.

The first general work which definitely represented the modern movement, however, was E. L. Thorndike's *Animal Intelligence*. This book appeared in 1898, and the experimental period in animal psychology may be said to begin with the twentieth century. Thorndike was interested in the ingenuity which animals display in the operation of mechanical devices, so, instead of collecting a lot of yarns about ponies unfastening gates and bulls ringing firebells, he brought his monkeys, dogs, and cats into the laboratory and studied them himself, under strictly controlled conditions. The animals were confined in boxes fastened by hooks and latches of various degrees of complexity, with food placed just outside. The behavior of each subject in this situation was carefully observed, and it is obvious that the information thus obtained is vastly more reliable than

that drawn from a collection of anecdotes from diverse and unknown sources.

Since the appearance of Thorndike's book the new science has progressed most rapidly in America, and a great number of experimental studies have been carried out by such men as Yerkes at Harvard, Carr at Chicago University, Watson at Johns Hopkins, Hunter at the University of Kansas, Porter at Clark University, and a number of others. These men are all professedly psychologists, but the innocent bystander sees no important distinction between their several vineyards and those cultivated by men like Loeb, Holmes, Jennings, Mast, Parker and others who are academically labelled biologists, physiologists and zoologists. Most of these men, in order to obtain leisure for research and time to think a bit, are forced to engage in teaching, and they have therefore developed schools of students, each after his own kind. Thus the results of many studies have been written up as theses for the doctor's degree, but most of them find their way soon or late into one of the more or less technical periodicals, such as the *Journal of Animal Behavior, the American Journal of Physiology, Psychobiology*, the *Journal of Comparative Psychology*, the *Behavior Monographs*, the *Journal of Comparative Neurology and Psychology*, the *Comparative Psychology Monographs*, the *Journal of Experimental Zoology*, and some others of lesser importance. Animal psychology, at present, has no esoteric and formidable terminology comparable to those employed in physics and chemistry, so that

practically all of these papers (except such as involve mathematical and statistical considerations) are quite intelligible to the general reader. The chief difficulty is to obtain access to the files of the journals, which are, outside of the larger cities, to be found only in college and university libraries.

A very convenient handbook is John B. Watson's *Behavior*, which appeared in 1914. This book not only presents in condensed form the results of the more important investigations, but is one of the earliest and best expositions of the behavioristic point of view—of which more anon. Another very valuable survey of the entire field of animal psychology, containing summaries of nearly all important studies published prior to 1917, is found in Margaret Floy Washburn's *The Animal Mind*—one of the very best reference books obtainable. *Mind in Animals*, by E. M. Smith, published in 1915, is a very useful little book, and perhaps a better introduction to the science than the larger works of Washburn and Watson.

If one is seriously interested in animal psychology he can, by reading these three books and dipping occasionally into the journals, obtain a very fair idea of the present status of our science. If his interest persist he may keep himself *au courant* by reading the brief survey of the year's progress which appears annually (usually in the August or September issue) of a periodical called the *Psychological Bulletin*.

The literature which I have mentioned deals

largely with the investigation of specific problems, and not to any great extent with theoretical considerations. This is as it should be, but it may be as well to outline briefly the two fundamental positions maintained by students of animal activities. To put the matter very bluntly: One school holds that animals have minds, comparable in a measure to our own, and the other school contends that they have not. This difference in fundamental assumption underlies all the literature of the science and is at the basis of all such questions as *Do animals reason?* and the like.

Montaigne, writing about 1580, was a celebrated defender of the view that animals have some sort of consciousness which may be interpreted in terms of conscious human experience. "The Swallowes which at the approach of springtime we see to pry, to search and ferret all the corners of our houses; is it without judgment that they seek, or without discretion that they chuse from out a thousand places, that which is fittest for them, to build their nests and lodgings? . . . Why doth the Spider spin her artificiall web thicke in one place and thinne in another? And now useth one, and now another knot, except that she hath an imaginary kind of deliberation, forethought and conclusion?" It is a most certain folly, says Montaigne, to ascribe all this behavior to mere instinct, or to "a kinde of unknowne, naturall and servile inclination." And elsewhere he says that "even brute beasts . . . are seen to be subject to the powers of imagination; witnesse some Dogs . . . whom we

ordinarily see to startle and bark in their sleep."

Montaigne's inclination to humanize animals was not taken very seriously until the time of the Darwinians, who wished, in Darwin's own words, "to show that there is no fundamental difference between man and the higher mammals in their mental faculties." Some of the wildest tales quoted by Romanes and other evolutionists were doubtless inspired by the desire to confound the enemies of the evolutionary theory, and to spread the gospel of Darwinism. At present, of course, the situation is very different; the Darwinian position is now very generally accepted by scientists everywhere, and, except for an occasional reactionary like the Jesuit Wasmann, the whole question of evolution is a dead issue.

However, there are still many investigators who, for one reason or another, ascribe consciousness to many if not all forms of animal life. Claparede says that no man really knows that even his fellow men have minds, but he infers that they have because their actions are similar to his own—and adds that we may rely upon the same sort of inference in the case of the lower animals. "The youth who finds himself drawn to medical studies, or he who is attracted to botany, can no more account for his profoundest aspirations that the beetle which runs to the odor of a dead animal or the butterfly invited by the flowers; and if the first shows a certain feeling corresponding to these secret states of the organism (a feeling of 'predilection' for such a career, etc.) how

can we deny to the second analogous states of consciousness?"

Forel, a Swiss entomologist, has made a special study of the social insects such as bees and ants, and thinks that he has indubitable evidence of "the existence of memory, associations of sensory images, perception, attention, habits, simple powers of inference from analogy, the utilization of individual experience, and hence distinct, though feeble, plastic individual deliberations or adaptations."

Wasmann has also carried out some very extensive investigations of ant behavior, and has no patience with any theory which denies a certain kind of mental experience to insects, but he is even more bitter against the view that they are endowed with an intelligence in any way comparable to our own. Wasmann is, among other things, a Catholic priest. He is also, since the death of Fabre, practically the only anti-evolutionist in the field. One gets a very fair idea of his position in the concluding chapter of his book on *The Psychology of Ants*, the English version of which was published at St. Louis in 1905.

"Of course, the results of our study are very different from, and indeed altogether contrary to, the aprioristic postulates of modern evolutionism, according to which man is nothing but the highest brute, and human society but a gradual evolution from that of the higher mammals. But scientific research cannot be hampered by such aprioristic theories; if they are incompatible with facts, they are aban-

doned. It is an undeniable fact, that between the soul of man and that of the brute there yawns a chasm which cannot be bridged over by any evolutionistic speculation. *Man is, as a matter of fact, the only being in the visible universe who is gifted with reason, with a spiritual soul, and with morality.* On account of the essential difference between sensitive and spiritual life, it is simply impossible that in the course of nature an animal should ever develop into man."

Therefore, continues Wasmann, we must crush this false psychology which brutalizes and demoralizes the human race, identifies the sexual impulse of the brute with human conjugal love, and the care of the young among animals with parental love in man. These teachings can lead to nothing but immorality and irreligion, and such men as Alfred Brehm and Ludwig Büchner have actually come forward as apostles of free love. "Every well-meaning naturalist, therefore, ought resolutely to oppose these unprincipled doings of this so-called popular psychology. . . *Those who humanize the animal not only trifle with scientific psychology, but they also drag into the mire the dignity of man.* . . If the moral principles of Brehm and Buchner should later on become the common property of humanity, then the society of the future from the highest to the lowest, would resemble a herd of unreasonable animals, whose 'spiritual life' would consist in the unbridled gratification of the meanest lusts and passions. Hence our concluding appeal: *Do away with all books,*

pamphlets and periodicals, whose only purpose is to raise the brute to the level of man!"

William Morton Wheeler, of Harvard University, whose *Ants* is probably the best book on the subject ever written, holds a very similar opinion as to the presence of mind in animals, although he shows no interest in the theological or ethical aspects of the problem. "After a patient, and, as I believe, unprejudiced study of ants, I have reached the same conclusions as Florel, Wasmann, and others, namely, that these insects show unequivocal signs of possessing both feelings and impulses. In my opinion they experience both anger and fear, both affections and aversions, elation and depression in a simple blind form, that is, without anything like the complex psychic accompaniment which these emotions arouse in us."

Miss Washburn reviews the opinions of all these defenders of the animal mind, and reaffirms her own faith in introspection as a method in human psychology. She believes that "we can....dispassionately and scientifically observe what goes on in our own consciousness when we receive certain stimuli and make certain movements. Further, we can, by the use of the same kind of inference from one case to another similar case, upon which all scientific generalization is based, infer that when a being whose structure resembles ours receives the same stimulus that affects us and moves in the same way as a result, he has an inner experience which resembles our own. Finally, we may extend this in-

ference to the lower animals, with proper safeguards, just as far as they present resemblances in structure and behavior to ourselves. Our object in this book will always be the interpretation of the inner aspect of the behavior of animals; we shall be interested in what animals do only as it throws light upon what they feel. To the true psychologist, no challenge is so enticing as that presented by the problem of how it feels to be another person or another animal; and though we must sometimes give up the problem in despair, yet we have also our successes. We have wonderfully advanced, within the last twenty-five years, in knowledge as to how the world looks from the point of view of our brother animals."

So much for the mind-in-animal people. The other school, which holds that animals do not experience sensations, feelings, and other conscious phenomena comparable to those credited to human beings, is well represented by Rene Descartes, who expressed his opinion about 1645. As far as the lower animals are concerned, Descartes was an outspoken mechanist; animals, he said, are mere automata, without feelings or sensations, and the death-scream of a dog on the dissecting-table is merely the crash of disintegrating machinery. Animals "act by force of nature and by springs . . . As for the understanding or thought attributed by Montaigne and others to brutes, I cannot hold their opinion. . . . Doubtless when the swallows come in the spring they act in that like clocks. And all that the honey bees do is of the same nature."

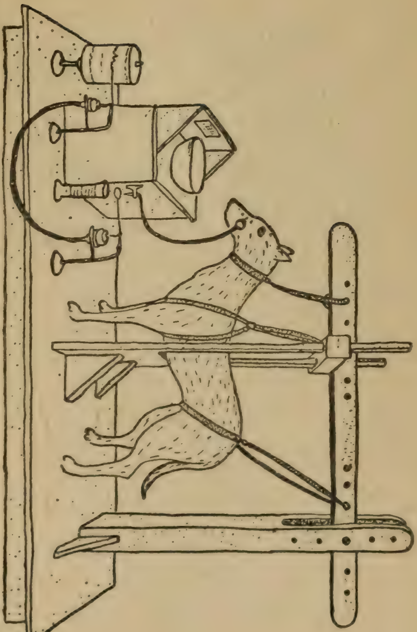

Fig. 1. The Pavlov Conditioned Reflex Apparatus. Saliva flows from the dog's cheek through a tube, and each drop tips a lever before falling into a graduated glass tube. The number of drops and their distribution in time are recorded upon the rotating drum at the extreme left; the total amount of saliva is retained in the graduate.

The doctrine of Descartes was preached by several of the materialistic philosophers early in the nineteenth century, but its chief defenders today are found among the physiologists and experimental psychologists. A. Bethe, an eminent German neurologist, has made some very significant studies of the behavior of bees. He explains all the complicated social life of these insects, which has been compared to our own civilization, as a result of reflex responses to chemical stimulation, probably unaccompanied by any conscious process whatever. "Psychic qualities," he says, "cannot be demonstrated. Even what we call sensation is known to each man only in himself, since it is something subjective. We possess the capacity of modifying our behavior (i. e. of learning), and every one knows from his own experience that psychic qualities play a part connected with this modifying process. Every statement that another being possesses psychic qualities is a conclusion from analogy, not a certainty; it is a matter of faith. If one wishes to draw this analogical inference, it should be made where the capacity for modification can be shown. If this is lacking, there is not the slightest scientific justification for assuming psychic qualities. They *may* exist, but there is no probability of it, and hence science should deny them. . . . Chemo-physical processes and their consequences; that is, the objective aspect of psychic phenomena, and these alone, should be the object of scientific investigation."

Von Uexkull, a celebrated physiologist, ridiculed the notion that animals have sensation,

memory, reflection and so on, and denied comparative psychology the right to call itself a science. Conscious states may exist, of course, but it is not at all probable, and it is little less than lunacy to assume their existence in the absence of any evidence whatever. And Ziegler, Nuel, and Von Beer agreed with him.

Von Beer, indeed, joined with Bethe and Von Uexkull in writing a remarkable paper entitled *Proposals for an Objectifying Nomenclature in the Physiology of the Nervous System*, which advocated the ruthless elimination of all such terms as sensation, memory, learning, and others which seem to imply that animals have minds. Instead of sense-organ they would say receptor, and substitute photoreception for vision, phonoreception for hearing, and so on. This paper was published in German in 1899, but seems to have attracted little attention at the time.

Jacques Loeb, another very famous physiologist, who died recently, always denied that animals have minds, and maintained that animal response is essentially identical with the activities of plants. Living beings are merely machines—complex chemical compounds—and their responses are nothing more than physicochemical reactions to the environment. There is no essential difference between living and non-living matter, and science will ultimately reduce all behavior to physical and chemical formulae. The student of animal behavior should study the physics and chemistry of living tissue, and not waste his time chattering about states of consciousness.

John B. Watson, in his book on *Behavior*, published in 1914, accepts the general position of the mechanists that there is no such thing as mind in the ordinary sense of the term; anyway, if there is such a thing, it is not open to scientific investigation. It is true that an animal can be taught to go to a red light and avoid a green one, but it is of no use to ask, as the psychologist is so prone to do "Does the animal see those two lights as the human being does, i. e., as two distinct colors, or does it see them as two greys differing in brightness, as do the totally color-blind?" Watson, as a behaviorist and objectivist, would ask instead "Is my animal responding upon the basis of the difference in intensity between the two stimuli, or upon the difference in wave-length?" and make no attempt to describe the animal's response in terms of his own experiences. Most workers in animal psychology today accept Watson's behaviorism as a method, and express their results in objective or behavioristic terms, even though they may not accept the implications of behaviorism in human psychology.

In this chapter I have attempted to indicate briefly the literature of animal psychology, to describe and compare the two methods of investigation, and to outline the two fundamental theoretical positions maintained by workers in this field. The rest of the booklet is devoted to the enumeration of some of the chief problems of our science, and the description of some of the attempts which have been made to solve them. As Watson has said, the vast majority of problems in both human and animal psy-

chology may be grouped under one or another of three heads. The first may be called *Sensory Discrimination*, the second *Instinctive Reactions*, and the third *Habit Formation*. This is the best classification that I have been able to find, and I have used it in this book.

CHAPTER II

SENSORY DISCRIMINATION

The sense-organs of animals have long been studied by the zoologists and physiologists, and a great deal of work with sensory responses has been done recently by comparative psychologists like Watson, Hunter, Yerkes, and Lashley. We probably know more about sensory processes than about any of the other matters treated in this booklet, but because the field belongs partly to the biologists, and because I regard the studies in habit-formation as of greater import, I have emphasized the latter topic at the expense of the others.

There are three principal methods of studying the sensitivity of an animal's receptors. One is the *method of Galton*, who, when he became interested in hearing in animals, walked through the London zoological gardens tooting a whistle concealed in his pocket. If an animal started, or turned its head, or pricked up its ears, Galton concluded that it heard the whistle. This method is a good one as far as it goes, but there are too many factors which cannot be controlled. The procedure most used in Amer-

ica is known as the *training method*, of which the following is a fair illustration. If one wishes to find out whether a rat can distinguish colors, he has only to place the animal in a discrimination-box, fitted, let us say, with two lights of equal intensity, one red and one green. Now, when the red light is showing, the rat obtains food at the right side of the box; when the green light is turned on, the food is found at the left. If the animal forms the habit of running always in the proper direction we conclude that it is able to distinguish between red and green lights. A third ingenious method is that of the *conditioned reflex*, a method devised by Pavlov, a Russian physiologist, about 1900. The salivary ducts of a dog, for example, are sectioned so that the saliva runs out into a glass tube, and this occurs whenever the animal sees or smells food. Now, if a sound, or a light, or some other stimulus is presented along with the sight or smell of food, it comes in time to cause a flow of saliva even in the absence of the food-stimulus. Perhaps a very dim light has no apparent effect upon the animal—we do not know whether he can see it or not. But if he can be so trained that the presentation of this light produces some reflex response such as a flow of saliva, there can be no doubt that his eyes are sensitive to it.

Color vision, or sensitivity to wave-length, has been studied to a very considerable extent, but most of the earlier experiments were inconclusive because there was no way to keep the intensity or brightness-values constant. Even a totally color-blind man may distinguish be-

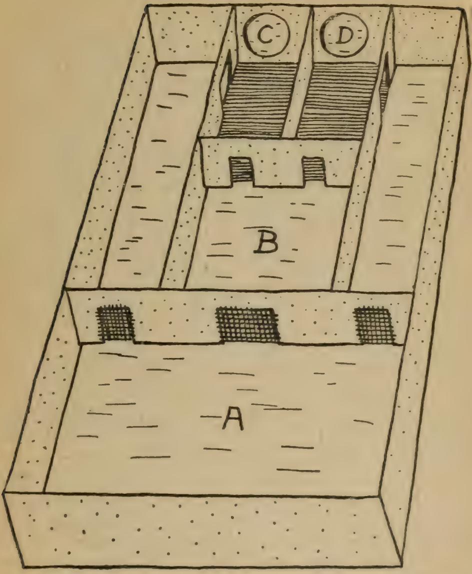

Fig. 2. The Yerkes Discrimination Box. This apparatus **has been widely** used in the study of sensory reactions. The **animal introduced** at A passes through the central door into the **chamber** B. The task now is to choose between two stimuli (colored lights, **let** us say) at C and D, and to return to A by one of the side alleys. Correct responses are rewarded with food at A, and **incorrect** choices punished by slight electrical shocks in the **two** alleys. If an animal can be taught to run toward the **red light and avoid** the green, **it is assumed that the retinae are sensitive to color differences.**

tween red and green papers if the red is always the darker of the two. Thus Nagel, in 1896, trained his dog to distinguish between red, blue, yellow, and green balls, but Nicolai, working in 1907 with a monochromatic light apparatus which gave lights of different colors but equal intensities, found that dogs are apparently color-blind. Watson has shown that monkeys are sensitive to colors, but no such evidence has been forthcoming in the case of cats, rats, mice and rabbits. Lashley, in 1916, demonstrated color-sensitivity in chickens, and it is probable that the same is true of other birds, but definite evidence is lacking. Bauer and von Frische think that fish discriminate between colored objects, but Hess, a very careful investigator, has failed to find any evidence for this view.

The hearing of animals has been the subject of a great deal of experimental study. Kalischer, in 1907, trained his dog to respond differently to high and low tones, and found evidence of a very high sensitivity to pitch differences. However, after the sensitive parts of both ears were removed, the dog reacted exactly as it had before the operation, showing that something was certainly amiss with the experimental conditions. Johnson, in 1909, repeated Kalischer's experiments, but used *blind* dogs, and his results completely discredited Kalischer's work. The blind dogs failed to respond to the sounds, and it is evident that Kalischer's animals were responding to some visual cues. Selionyi, however, in 1907, studied the problem by means of Pavlov's method, and his results seem to sup-

port Kalischer. Watson intimates that Selionyi's experiments were badly controlled, but still "it would not be safe to conclude until further experimentation has been undertaken that the dog is insensitive to pitch difference." Shepherd, in 1912, demonstrated sound-discrimination in cats, and found that both cats and raccoons learn to respond to articulate sounds. Cole reported that his raccoons were able to distinguish between the high and low tones of a French harp. Yerkes showed that adult dancing mice are quite insensitive to hand-claps, shouts, whistles, etc., but that the young dancers have a rather acute sense of hearing. Hunter, in 1915, found that ordinary white rats are sensitive to noises but do not react to tones. Pfungst discovered that cavalry horses do not respond properly to bugle-calls, but are reacting to some other cues, and are probably involuntarily directed by their riders. Monkeys, according to Shepherd, are quite sensitive to the pitch-differences of a German mouth-organ.

Several species of birds have been shown to possess a rather acute sensitivity to sound. Frogs make no visible response to loud noises such as shouts and pistol-shots, but careful laboratory studies have demonstrated that they are sensitive to sound. Yerkes mounted a frog in a sort of saddle so that its legs hung free—when the head is touched the leg jerks upward, and the extent of the jerk was measured. It was found that when an electric bell was sounded just before the tactual stimulus was given, the vigor and extent of the movement was considerably increased, proving that there

is some sort of auditory sensitivity. Bateson, Kreidl, Lee and others have reported that fish cannot hear sounds, although they are profoundly affected by jars. Parker, however, found that fish in which the auditory nerve has been severed do not respond normally, and contends that they certainly do hear something or other.

Very few satisfactory studies have been made in the field of smell, because of the great difficulty in controlling the stimuli. It is easy enough to turn off an electric light, or to stop the vibration of a tuning-fork, but the manipulation of odors is another matter. The hunting behavior of dogs, for example, is well known but most difficult of explanation. It is generally believed that a common rabbit-hound will often follow a trail twelve hours old, and bloodhounds are credited with an even more acute sense of smell. This extraordinary sensory acuteness is difficult of belief and comprehension, but still more difficult to understand is the dog's apparent ability to pick out one particular component from a fusion of different odors. Romanes, in 1887, reported an experiment with a setter bitch which is a classical illustration of this analytic power. Twelve men walked along in Indian file, taking particular pains to step directly in each other's tracks; the bitch, however, picked out her master's trail and followed it, although it was overlaid by eleven others.

Another thing to be noted is the dog's ability to follow a trail in the same direction taken by the quarry, and to avoid back-tracking. It is usually assumed that this is a mere matter

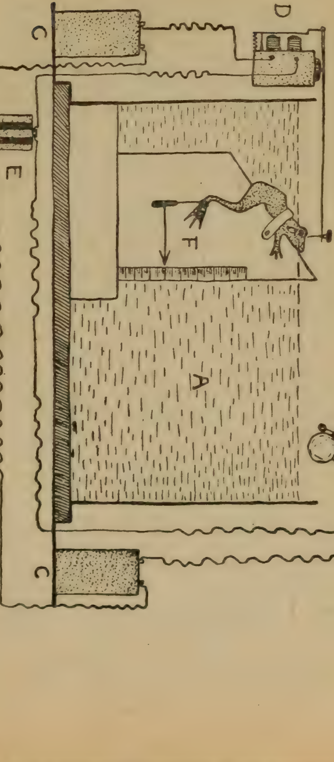

Fig. 3. Yerkes' Apparatus for Testing Hearing in Frogs. A, aquarium; B, electric bell; C, battery; D, tactual stimulus apparatus; E, hand key for giving stimulus; F, scale which registers the extent of leg-reaction. (Adapted from Yerkes.)

of intensity—the odor of each particular footprint being just a trifle more intense than the one behind it, and just a little less intense than the one ahead of it. Johnson, in 1914, pointed out a number of serious objections to this assumption. In the first place, the intensity difference between two points in the trail of a rabbit depends upon the time which has elapsed between the rabbit's passage of the two points. Now, if the dog follows along at the same speed as the rabbit there is no intensity difference at all, and unless the dog is much faster than the rabbit the difference is negligible. Another view is that the dog distinguishes between the odor of the heels and that of the toe, and so avoids back-tracking by sensitivity to the form of the tracks. However, as Johnson says, if this were true one could evade the bloodhound simply by walking backward, which is not the case.

Dr. P. W. Cobb thinks that perhaps the sense of direction may be simply a matter of trailing ground-substances. Thus, if the trail crosses a mint-bed, the tracks made beyond the bed will carry the odor of mint, while those made before the bed was traversed will have no such odor. Even Johnson admits that this ingenious hypothesis impresses him as being valuable, "although it does not afford a complete explanation of the facts as variously alleged."

Birds have well developed olfactory organs, but there is at present not a scrap of evidence that they perceive odors at all. Many persons have claimed that vultures and buzzards find carrion by means of the odor, but there is no real evidence that this is the case. Audubon,

as long ago as 1835, discovered that vultures do not find carrion hidden from their eyes, and that they do find and tear imitation carrion (stuffed deer and the like) which has no odor at all. Darwin worked with condors, and arrived at a similar conclusion. The experiments of C. W. Beebe, who used vultures as subjects, were less conclusive, and R. M. Strong, who saw a turkey-buzzard peering down a hole, and later found a dead turtle inside, is almost inclined to ascribe the reaction to olfactory stimuli.

CHAPTER III

INSTINCTIVE RESPONSES

Instinctive responses are those which can be made without practice—which do not have to be learned. The simplest instinctive responses are those of the unicellular animals, and are known as *tropisms*. The tropistic reactions are purely mechanical, and are classified according to the stimuli which set them off: phototropism is a response to light, geotropism to gravity, chemotropism to chemical stimulation, and so on. There is no evidence of learning or habit-formation among the one-celled animals, but they are sensitive to all the classes of stimuli which affect the higher animals, and their tropistic behavior is the prototype of all more complex responses. If an instinctive response is a trifle more complicated than a simple tropism it is known as a *reflex*, particularly if it affects only a single organ or segment of the body.

Jacques Loeb has maintained that all animal behavior is made up of tropisms and reflexes; animals have no minds, all the acts of living beings are purely mechanical, and the reactions of plants and animals do not essentially differ. What an organism does depends upon the relative intensity of the stimulus at different points upon the body surface. A phototropic animal moves toward the light because the muscles on one side of the body are stimulated more intensely and react more strongly than those on the other side, and thus turn the organism's head end toward the stimulus. If the muscles on the least-stimulated side react more strongly than the others, then the body of the animal is swung about with its tail toward the stimulus. Thus the notion that animals are guided by some purpose or other (the search for food or mates) has given place to Loeb's mechanistic view, just as the teliological theory of planetary movement fell before Newton's gravitation hypothesis. The *mechanical dog* constructed by John Hays Hammond is a striking illustration of Loeb's general point of view. This contrivance is positively phototropic, and will follow a man with a lantern through a dark street. The eyes are two lenses, with a wooden nose between so that one eye is always more illumined than the other unless the dog is pointed directly toward the light. Two retinae are made of selenium (the galvanic resistance of which is affected by light) so that when one gets more light than the other the electric energy (supplied by batteries inside) which moves the wheels (legs) does not flow sym-

metrically to the steering apparatus, and the thing turns toward the light. Loeb thinks that the activities of all animals, from amoeba to man, are of this mechanical type.

Some of the current problems in the study of instincts are the order of appearance, the degree of accuracy or perfection, the influence of habit, loss through disuse, waning with age, etc. Our present ignorance of all these matters is truly lamentable. Although a number of incidental observations on the instinctive reactions of mammals and birds have been recorded, they are of no great value, and, as Watson says, "it is highly improbable that any of us could describe in a really helpful way the daily routine of the domestic fowl or the dog." Watson's own study of the noddy tern, with particular regard to the homing instinct, is one of the most valuable investigations of the sort ever made. He found that these birds actually return to their homes over a thousand miles of absolutely strange water. They seem to locate nearby objects by vision, but this is out of the question in the longer flights, no matter what sort of telescopic eye-sight they might have, by reason of the curvature of the earth. It is not a matter of smell, either, because birds with plugged nostrils seem to fly as well as the others.

F. S. Breed, in 1911, investigated the instinctive responses of chicks, such as the pecking and drinking reaction, and was astonished at the inaccuracy displayed at first, before the behavior was modified by maturation and habit. Others have remarked the fallibility and im-

perfection of instincts; the classic example being reported by Pearse, who found that the sexual behavior of crawfish is so blundering that the males actually cannot distinguish between the sexes, but "turn over every crawfish that comes their way, and often attempt to copulate with individuals of their own sex." Shepherd and Breed, in 1913, tried to find out how much of the chicks' improvement was due to maturation and how much to practice. Berry, in 1908, did some experimental work with kittens, and published the conclusion that there is no inherent tendency to kill mice, but that the young animals learn this response by imitating their parents. Berry's work was discredited, however, by Yerkes and Bloomfield, who found the mouse-killing behavior in kittens five weeks old which had never seen their elders do the trick. Yerkes, in 1913, tested the heritability of wildness, savageness and timidity in rats, and holds that these are clearly inherited instinctive behavior complexes.

Many other studies of instinctive behavior have been carried out, but because of limited space they cannot be reviewed here. The subject of habit-formation is, in my judgment, of greater interest and importance, and the rest of this booklet is devoted to that subject.

CHAPTER IV
HABIT FORMATION

The problems connected with habit-formation are problems of learning and forgetting, and it is with these that the pedagogues and educa-

tional psychologists are chiefly concerned. As W. S. Hunter says: "It is very important to know what the laws of learning are and how conditions may be best adapted to secure the highest efficiency. Is it more economical to give one trial a day, or two, or three? Should one learn a task in parts or should one learn it as a whole, if economy of effort is to be secured? Does learning ability vary with sex and age? Do habits interfere with each other, and can efficiency in one task improve ability in another? How does loss of retention proceed? Is it most rapid at first and slower toward the last? These and many other problems of great practical value can be answered as well, if not better, by tests upon animals than by tests upon humans, for with animals we more readily control motives, prepossessions, and modes of living, and we also secure more convenient material."

One of the first experimental studies of animal learning was Thorndike's work with dogs and cats, published in 1898. He placed his hungry animals in boxes, so arranged that they could obtain food only by opening the door, which was accomplished by pulling wires, clawing at buttons, raising a thumb-latch, and so on. In every case the animals made all sorts of random and frantic movements in their efforts to escape, various parts of the cage being attacked with no apparent discrimination, the release mechanism was often completely overlooked, and was usually operated at last by a chance or accidental move-

ment. The next time the animal was placed in the box it did not appear to profit in the least by its previous experience, but struggled wildly about as before, until the proper movement was again made seemingly by chance. Finally, after a large number of trials, the animal's attention was confined to the part of the box in which the latch was located, and still later the appropriate manipulation was made immediately, the mere sight of the mechanism being sufficient to set off the movements necessary to its operation. Thorndike called this process *trial-and-error learning.* and scouted the current notion that it evidenced any particular intelligence or reasoning power. His records were simply notations as to the time required for the animal to effect its escape, and curves were platted from these data to represent the progress of the learning. Thorndike asserts that "the gradual slope of the time-curve . . . shows the absence of reasoning," and goes on to interpret this gradual slope as "representing the wearing smooth of a path in the brain, not the decisions of a rational consciousness." Thorndike's view seems to have been that a sudden vertical descent in the time-curve would be evidence for the presence of reason or intelligence in the animal. In 1901, when he tried monkeys in his problem-boxes, he found that the graphs did show this sudden vertical descent, "being unanimous, save in the very hardest, in showing a process of sudden acquisition by a rapid, often apparently instantaneous, abandonment of the

unsuccessful movements and a selection of appropriate ones which rivals in suddenness the selections made by human performers." Even then, however, Thorndike hesitated to conclude that his monkeys used *ideas* in the process, but rather accounted for their advantage over dogs and cats by superior eye-sight, better use of the hands, and similar purely physical considerations.

Another point which, in the case of the cats at least, indicated the absence of ideas was the fact that these animals could be trained to associate release with licking or scratching themselves, or some other act which had no rational connection with the door, quite as easily as they could be taught to escape by manipulating the locks and latches. Still another thing was the failure to learn by being *put through* a particular activity. Some of Thorndike's cats formed the habit, at the beginning of the experiment, of entering the cage without assistance, and it was suggested that they may have come to associate the idea of being in the box with the feeding which rewarded their escape. So he tried putting some other cats into the box from the top, and found that the tendency to return was lacking. From this he concluded that the association involves muscular behavior rather than ideas—"the impulse is the *sine qua non* of the association."

One of the most interesting results of Thorndike's work with monkeys was his discovery that, contrary to popular conceptions of these animals, imitation seems to play no part in

their learning. After attracting the attention of the monkey, he opened the box himself very slowly, taking care to emphasize all the essential movements involved. This was repeated several times, but the monkey did not profit in the least by this example; those who watched Thorndike's performance did not learn any more rapidly than the others. They did not learn anything by watching their fellows, either. A monkey which had learned the manipulation by the trial-and-error method was allowed to open the box in full view of the untrained animals, but they did not master the task any sooner than did other monkeys which had no opportunity for imitation.

A. J. Kinnaman, who worked with monkeys at Clark University in 1902, could not agree with Thorndike about the influence of imitation. One of his problem-boxes was so arranged that food was obtained by pulling out a plug, and the female monkey failed to solve the problem and gave up in apparent despair. The male monkey came along and pulled the plug without difficulty, the female watching meanwhile. When the box was set again the female rushed immediately to the plug, pulled as she had seen the other monkey do, and got the food. She repeated this performance eight times. In another box, where the food was obtained by pressing down a lever, the female had failed to work the mechanism, but the male had mastered it easily. Later on the female was allowed to watch her lord and master press the lever and get the food, and she went in at once and followed his example.

Cole, in 1907, in a series of experiments with raccoons, found what he regarded as indubitable evidence that they imitated the experimenter. In testing their discrimination of color he used a card-displayer, which showed a number of colored cards, one at a time. One particular color always meant food, and when this color appeared the animal rushed to a box where it was fed. Now, in operating this card-displaying contrivance, Cole manipulated a lever which brought the cards into view, and he says that four of the animals, after watching him for some time, operated the lever themselves. Then, as soon as the proper color appeared, they presented themselves at the food-box to be fed.

H. B. Davis, in 1907, reported one case of doubtful imitation in a raccoon, but in most of his work found no trace of it. One of his animals was taught to jump high in the air to get food, and although all the others saw him do this many times, not one showed the least inclination to imitate the act.

Watson, in 1908, carried out a series of experiments with Rhesus monkeys, in order to examine the evidence for and against imitation. He says: "Two of the monkeys were quite friendly with each other. J was an adult monkey and B a young one. In a short time an attachment sprang up between these monkeys which persisted for several years. B was wild and restless and became excited when J was away from him. J on his part would run to B on certain cries and put his fore paws

around B's neck. B would nestle up to J and clasp him around the chest. . Under these conditions, it is quite evident that J's reactions influenced B's greatly. When J went to one part of the cage B followed. If, while sitting on the shelf, a pan of water or a bowl of milk was placed upon the ground, B would not come down to drink if J did not precede him. . . . Several types of problems were given these animals in the work upon imitation. (1) Drawing in food with a rake, animal to imitate the experimenter; (2) drawing in food with a cloth, animal to imitate the experimenter; (3) obtaining food from bottom of bottle by the use of a fork, animal to imitate the experimenter; (4) pushing out food from the middle of long glass cylinder by means of a stick, animal to imitate experimenter; (5) manipulation of old-fashioned latch, animal to imitate experimenter; (6) box with a door in top not held in place by any fastening, animal to pull open door by means of a handle, B to imitate J; (7) box with door in top held in place by a push button, B to imitate J. The monkeys very quickly form by their own unaided efforts such habits of manipulation, but so far as our observations went they were entirely uninfluenced by tuition." Watson, then, agrees in general with Thorndike that imitation is not a factor in the learning processes of these animals.

W. S. Berry, whose work was done at Harvard in 1908, said that his cats learned to open boxes, roll balls into holes, etc., by watching

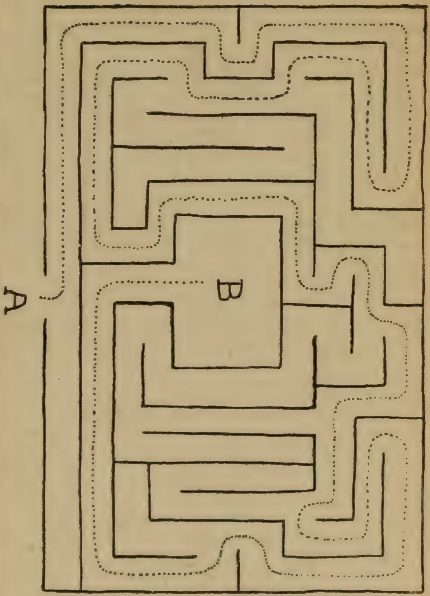

Fig. 4. The Hampton Court Maze. As constructed for use with rats the alleys are of wood, six inches in width and six inches high. A represents the entrance of the maze; the dotted line shows the true path; B is the box where the animal is fed.

the performance of trained animals of their own species. He even claimed, on the basis of his experiments with young kittens, that cats do not kill mice instinctively, but learn to do it by imitating their elders. Watson wrote a very harsh criticism of Berry's paper, so Yerkes and Blumfield, two of Berry's colleagues at Harvard, repeated the experiments. They found that when a kitten gets about five weeks old it develops an instinctive tendency to kill mice, no matter whether it has ever seen other cats hunting or not.

Lightner Witmer, in 1909, reported some apparently imitative responses of a chimpanzee named Peter, *the Monkey with a Mind*, in writing the letter W on a blackboard. He also speaks of another monkey which opened a door after seeing it opened by the experimenter. Watson, in criticising Witmer's work, says in part: "He maintains that this door was opened for the first time by the monkey. There is nothing in his paper that would show that he knew anything about the previous history of the monkey. Opening a door is one of the easiest things that a monkey does. Time and time again we have had them unhook doors, twist off wires which were wrapped around doors, break wires, take a round knob and turn it with the hind-feet while holding onto a nail in the wall with the fore-feet. All of these acts were readily learned without tuition and in the absence of the experimenter. It is very easy to be deceived about the accomplishments of an animal not under daily observation."

Haggerty, working with monkeys in the Bronx Zoo about 1909, used a number of simple mechanical devices, the operation of which opened doors or dropped food into the cages. His procedure was as follows: Each animal was given a fifteen-minute trial daily for fifteen days—some solved their problems, while others failed. Then each of the unsuccessful monkeys was allowed to watch one of the trained animals at work, and then given another trial himself, being allowed ten minutes, or even longer if he seemed about to succeed. No animal was considered as having failed to imitate until he had been tested one hundred times. Haggerty used eleven animals, and only two of them failed to exhibit some sort of imitative behavior. Watson says in commenting on this experiment that "Haggerty mentions the fact that the animals imitate best when not too thoroughly accustomed to one another. He states that these cases are cases of imitation of the inferential type. We are not able to agree with him. In the first place, his method of allowing the animals to have one hundred chances for imitation before considering tuition without effect must always give ambiguous results. With an animal as agile and as varied in his interests as the monkey, it is never possible to tell when a burst of activity will lead to the solution of the problem by the perseverance method. On the other hand, it is possible to assume that the only effect of the act of the imitatee was to set off two congenital forms of response on the part of the imitator.

The one was the following instinct—i. e., to go to the place where a group of monkeys had just been and to peer into the same holes that other monkeys had just peered into, etc.; and the other the tendency to attack moving objects first. The general perseverance method of the animal took care of the remaining factors."

Porter, in 1910, found evidence that several species of birds (sparrows, cow-birds, juncoes, orioles and crows) imitated each other in opening problem-boxes, but his method and his criterion of imitation are open to serious criticism. He simply put a bird which had not learned to open the box to work in company with one which had mastered the problem. If the untrained bird suddenly learned how to open the box it was assumed to have imitated the other.

W. T. Shepherd, who also worked in 1910, suspended a banana from the ceiling of his monkey-cage, so arranged that the monkeys could reach it only by pushing a horizontally pivoted rod under the banana, and climbing up on the rod. There seemed to be some slight evidence that one monkey learned the trick by watching another.

It will be seen from the above summary of the experimental work on imitation that the results are by no means harmonious. On the whole, however, as Watson says, "enough positive results have been obtained to show that in some way the process of learning is modified when certain forms of tuition are used. If further work were conducted along lines which are not anthropomorphic—if experimenters

would confine their work to determining just what the animals do without being concerned about the fact that they may be acting as human beings would under similar circumstances —the control of behavior by such methods in both human and animal work would more speedily be realized."

Watson, as long ago as 1902, concluded that young white rats form certain motor habits, such as maze-running and the manipulation of some problem-boxes, much more rapidly than the adults, "the general conclusion being that any habit which requires for its learning excess running, climbing, etc., could be learned by any rat at the age of about thirty-five days more rapidly than by the adult. On the other hand, those problems which depend for their solution upon control of movement can be learned by the adult more rapidly."

R. M. Yerkes, in 1907, in his work with the famous *dancing mice*, (a strain of mice having defective canals in the inner ear, which causes them to waltz about in a peculiar fashion) established the fact that thirty-day-old dancers form certain habits in a smaller number of trials than can older individuals. This ability seems to increase until the rat is about seven months old, and then gradually decreases. In certain discrimination-habits, such as choosing between white and black, young animals respond more rapidly to slight differences in the intensity of the stimuli.

J. R. Slonaker's paper on the *Normal Activity of the White Rat at Different Ages*, deals with the same problem from another angle. Slonaker

made no attempt to teach his animals anything, but simply confined them in cages fitted with a mechanical device which registered the movements of the rat each time it left the nest for food, water or exercise. He found that, in general, a rat is more active at about one hundred days old than at any other period in its life. Watson, in discussing Slonaker's work, points out that "the period of greatest activity as obtained by his method need not at all coincide with the period of greatest activity of the kind involved in attacks upon problem-boxes."

Helen B. Hubbert, in 1915, studied the relation of age to an animal's habit-forming ability. She used albino rats in a circular maze like that used by Watson. One hundred and twenty-two rats, whose ages varied between twenty-five and five hundred days, were taught the maze, and the average time, average number of trials required, and average distance traversed by each age group in the learning process were computed. Miss Hubbert found in general that "young rats learn the maze more readily than the old ones, the rapidity with which the habit may be formed decreasing with increase in age." Incidentally, her results indicated that, except in very young and very old rats, the males mastered the problem in less time than did the females. It was noted also that the animals seem to learn the maze quite as easily when the apparatus is in total darkness as when it is lighted. In concluding her paper she says that "if an analogy may be drawn between the learning ability of the rat and that of the human

subject, it may be seen that in general the old can learn a given problem as well as the young, although more effort is required to do so." This last does not necessarily follow by any means. As Oakland Maupin has pointed out, "it is impossible to determine what relation a sixty-day-old rat bears to a man, and besides, in a problem of this nature, there is no reason why one should not work with human beings, if results which have reference to them are desired. That will always be the best way to solve human problems. The contributions of animal psychology to human psychology, at least as far as learning is concerned, have been the development of a behavioristic point of view and a method of approach to certain problems for which human subjects are hard to secure."

G. C. Basset, in 1914, also made use of the Watson circular maze in an attempt to determine what relation exists between brain-weight and learning ability. Having possessed himself of some sixty inbred rats whose brains were slightly below the average in size, he found that these animals did not master the maze as readily as the normal rats. Tests in an inclined plane problem-box gave similar results. However, as Vincent and others have since pointed out, there has been some discussion as to whether the lesser ability was due to the decreasing brain-weight (which ceases to decrease after the fourth generation of inbreds) or to some other factor connected with the inbreeding. As Maupin said in 1923, "if Basset secured rats, during the inbreeding, which were

not inferior in brain weight to their species, this factor could easily have been controlled; if he did not, it would, of course, be impossible to determine what the *essential* cause of the decreased ability might have been."

Ada Yerkes, in 1916, studied Basset's problem, using rats in a maze of the modified Watson type. She worked with rats of the thirteenth inbred generation, and found that the male inbreds were slightly inferior to the ordinary rats, while the female inbreds, although behaving in an irregular and erratic fashion, learned the maze in less time than the normal females. Postmortem examination, however, revealed the fact that the inbreds of both sexes actually had larger brains than the ordinary stock rats, and as only sixteen animals were used altogether the results are not particularly conclusive anyway.

K. S. Lashley carried out a series of experiments in 1917 to determine the effects of certain drugs upon the ability to learn the maze. Among other things, he found that rats fed with caffeine learned rather more rapidly than normal animals, but that after the habit had once been acquired, further doses of caffeine seemed to decrease the accuracy of the performance.

Ada Hart Arlitt, in 1919, tried out the effect of alcohol, and reported that her alcoholic rats required more time, more trials, and made more errors in acquiring the maze-habit than the control group of teetotalers. She held also that "parental alcoholism resulted in the lessened speed of running and in rate of learn-

ing, even when the dose administered to the parent animals has been small and the feeding period short," and adds that the inferiority persists even unto the third (but not the fourth) generation.

Thorndike, in 1901, concluded on the basis of his experiments with dogs, cats and monkeys, that images and ideas do not enter into the learning processes of these animals. Several attempts have been made since to obtain positive evidence for imagery, but no very conclusive results have been obtained, and even Miss Washburn, in 1917, admitted that "ideas are very rare in the animal mind." We must face the fact that because man has images (and the Watsonian behaviorists deny even this) is no particular evidence for their existence in other animals, which can be established only by rigidly controlled experimental study.

Cole, in the course of his labors with raccoons in 1907, trained the animals to respond to a red card in his card-displayer if it had been preceded by white and blue cards; if the preceding cards were red, however, there was no response. Cole's position was that an animal could not learn this trick unless it "retains an image of the cards which just preceded red." However, he goes on, "I never completely inhibited the animals' tendency to start up on seeing white or blue, which were the precursors of the red which meant food. Thus the animals all anticipated red on seeing its precursors, which in itself seems good evidence of ideation. Many times, however, they turned back after starting at white or blue and looked for the

red, then climbed up once more, thus showing that the red was not a neglected element of the situation, but an expected color which they generally waited to see, but sometimes were too eager to wait for." Cole's arguments are rather impressive at the first hearing, and one of the most beautiful illustrations of the need for caution in these matters is found in the devastating results obtained by some of Cole's critics.

Gregg and McPheeters, working at Chicago University in 1913, repeated Cole's experiments, and found that the animals mastered the card-trick just as Cole had reported. They believed, however, that the cue was somehow given by the experimenter, so they hid behind a screen and operated the levers of the card-displayer by long strings—but the raccoons responded the same as ever. Finally it was discovered that when the proper card appeared McPheeters always reached for the food-box. It seems that McPheeters was a preacher, addicted to the round stiff cuffs of our sturdy fathers, and further, that the cuffs rattled every time the owner reached toward the food-box. The raccoons had merely learned to run to the box whenever they heard the clatter of the cuffs!

After McPheeters discarded his stiff cuffs the animals were completely baffled, all save one, who seemed to work as well as ever. Next the colored cards were taken off, so that nothing showed by the three plain wooden surfaces, but when the levers were worked in the old white-blue-red order the animal responded just as he always had, although *no colors were shown*. The experimenters now thought that the response might be a matter of odor, so the

whole apparatus was thoroughly scrubbed, but the raccoon seemed to make his choices as unerringly as before. Then someone suggested that the wooden levers perhaps differed a little in appearance, so they were taken off and changed about, but the raccoon was not bothered a bit; if the lever occupying the position of the original white card was worked first, and followed by those corresponding to the original blue and red levers, the animal did his stuff just as well as when Cole thought he was responding to red-white-and-blue images. At this point, however, a sheet of heavy glass was placed between the animal and the machine, and he *failed* ignominiously. The final conclusion of the matter was this: The levers were so pivoted that they were not all at exactly the same distance from the raccoon, which always extended his nose out to almost touch the lever before responding. The difference was not more than a quarter of an inch, but it was enough—the animal based his responses on the extent to which his neck-muscles were stretched as he examined the levers. So the whole thing was merely a matter of muscular sensation—kinesthesis—and visual sensations or images had nothing to do with it.

W. S. Hunter, in 1913, published his work on what he called the *delayed reaction*. Using rats, dogs, raccoons, and children as subjects, he flashed a light in one of three windows, and then, after a certain delay, required the animal to go to the place where the light had been; if the proper place was chosen the animal was fed. He found that rats were able to learn this stunt if the period of delay was not more

than ten seconds. Raccoons could *remember* for twenty-five seconds, dogs for five minutes, and children for at least twenty minutes. The rats and dogs simply *pointed themselves* at the proper window, and ran to it the moment they were released. Most of the raccoons did the same, but in some cases Hunter missed this overt orienting attitude, and concluded that they were responding to some sensory cue that could be lost and reinstated, thus serving as an image. He did not think it *was* an image, however, but preferred to speak of the process as sensory or imageless thought. The younger children used in the experiment responded precisely as the raccoons did; some of the older children, Hunter thinks, may have been guided by processes involving imagery. Many animal psychologists now agree with Hunter's views about this sensory thought business; R. M. Yerkes is the chief representative of the older view.

What is known as Yerkes' *mutiple choice* method of studying imagery in animals began with the work of G. V. Hamilton in 1911, but it has been developed at considerable length by Yerkes, who published his first results in 1914. The following description of the method is taken from Washburn: "The animal is offered the choice among a number of compartments. The number can be varied, and their position in space can be varied. Thus, if there are ten compartments, in the apparatus, only three of them may be used in a certain experiment, and these three may be situated in the middle or towards either end, so that no asso-

ciations will be formed with positions in space. Or in another experiment five of the compartments, in any part of the series, may be used. The compartments used in a given experiment have their entrance doors open. The problem may be varied in complexity by making the *right* compartment, the one whose entrance gives food, bear different relations to the rest. It may be the first compartment on the left, the first compartment on the right, the second on the left, the second on the right, the middle compartment, and so on. After an animal has proved its ability to learn a simple problem, such as *first on the right*, it may be advanced to a more complex one, such as *second on the left*." This method has been used with birds, rats, pigs, monkeys, apes and humans. Many animals of all these species learned to respond correctly, and Yerkes thinks that this is certain evidence that they have images. If the animals were really reacting to *relation* Yerkes is doubtless right, but there is no very satisfactory evidence that this is the case. A dog can be trained to pick up a stick by the middle, or by either end, and the use of a longer or shorter stick will have no great effect upon his behavior. This does not prove by any means that the dog has ideas or images of relation.

In 1917 Yerkes happened to observe that as a monkey went into a problem-box he stepped on a tack, and as he came out of the box he pulled the tack out of his foot. The next time the monkey came out of that box he stopped and examined the injured foot attentively.

Yerkes actually published this as evidence that the monkey had images, but his paper was ridiculed by Hunter, Watson, Carr and others, and has never been taken very seriously by anybody.

The general principles involved in training animals and children seem to be identical, and in many cases it is more convenient to work with the former. Just as many important facts about human physiology have been brought to light by work upon the bodies of the lower animals, so doubtless much of the future progress of educational psychology will be due directly to the labors of the comparative psychologist.

Ulrich, in 1915, by an extensive study of the rat in the problem-box, discovered that a habit may be learned in fewer trials if the effort is distributed in time; that is, one trial a day for ten days is better than two trials a day for five days, or ten trials in one day. The same general results were obtained when mazes were used, and when three different habits were acquired at the same time. In general, the less frequent the trials are the smaller the number of trials necessary, but, of course, the number of days required is greater. If one wishes to conserve his energy, one trial per day is best; if he wants to save time at the expense of effort, more frequent trials are in order.

Lashley, in 1917, studied the records of rats which had learned the circular maze by the method of five trials per day. He found fewer duplicate errors in the trials separated by a twenty-four hour interval—that is, in the last

Fig. 5. The Watson Circular Maze. The animal starts at the entrance A, and must run to the food-box at B. Punishment is not ordinarily used. The true path is indicated in the diagram by a dotted line.

trial on one day and the first on the next—than in the other parts of the series.

Pechstein, in the same year, used both rats and humans in a modified Hampton Court maze in the attempt to find out whether it is better to attack the problem as a whole, or to master one part of it thoroughly and then pass on to another part, and so on until the entire maze is learned. This is perhaps analogous to the question about memorizing a poem—should one learn it stanza by stanza, or try to get the whole thing at once? Pechstein built his maze so that it could be divided into four parts. Learning by the *whole* method consisted in running the entire maze each time until the perfect habit was acquired. In the *pure part* method the four sections were learned one after another, and finally combined so that the entire maze could be run without error. He found that, with the rats, the whole method required fewer trials but more time, and involved more errors. With human subjects the whole method was best in every way—fewer trials, shorter time, and fewer errors.

The next move which occurred to Pechstein was to devise four modifications of the part method. In the *direct repetitive* method the subject learned the first section of the maze, and then the first and second sections together. When this was done, the third section was added to the first and second, and at last the fourth section was added to the other three. The *reversed repetitive* method was the same, except that the maze was learned backward—fourth section, then fourth and third, next the

fourth, third and second, and at last the fourth, third, second and first. In the *progressive part* method the subject learned the first and second sections separately, and then the two together; then he mastered section three separately, after which he combined the first, second and third. Finally he learned the fourth section alone, and then ran them all together —first, second, third, and fourth. The *elaborative part* method required the mastery of each section separately; when each had been learned the subject ran the whole maze backward, and then finally in the ordinary manner. Pechstein concluded that every one of his new modified part methods were superior to the *whole method* or the *pure part* method, and thinks the *progressive part* system the best of all.

Dashiell, in 1920, put both rats and humans into his mazes in the effort to answer the following question: In learning two habits, is it best to master one completely before tackling the other, or to study them alternately until both are acquired? Using two different mazes, he trained one-half of his subjects in one until they were perfect, and then required them to learn the other also. The other subjects were trained first in one maze and then in the other, the alternative training being continued until both habits were learned. In every case, in every way, the former method was found superior.

It has been found that the formation and retention of certain habits may aid or hinder the acquisition and retention of others; in the

former case the phenomenon is called the *transfer of training*, while in the latter it is known as *habit interference.* The problem of transfer has particular significance in the training of animals, and was long ago discussed by Yerkes, Watson and Yoakum, but no important experimental work appeared until comparatively recent years.

Hunter, in 1911, found that pigeons which had mastered one maze made more errors in learning a second labyrinth than birds which had no training at all—a clear case of interference. Bogardus and Henke, in the same year, used rats in various mazes, and found plenty of evidence for interference between the various habits. Hunter and Yarbrough, using rats as subjects, studied auditory-motor habits and found that the formation of one simple habit certainly slows up the learning of another. Contrary to popular opinion, however, the second habit does not interfere in the least with the retention of the first. Pearce, using visual instead of auditory stimuli, reported such a tremendous amount of interference that many of her rats were unable to learn the second habit at all.

Webb, in 1917, trained both rats and humans in a variety of mazes. In every experiment he found a marked transfer effect, and no interference whatever. He thought, in opposition to Hunter and Yarbrough, that there was some slight evidence for retroactive inhibition —that is, that the acquisition of a second habit interfered with the retention of the first.

Wylie, in 1919, used a discrimination-box in-

stead of the maze, and a large number of rats were trained in different situations. The following summary is from a review by Oakland Maupin: "In some instances the animal was taught to choose light, which was given in irregular order from right to left. When this habit was established, sound was substituted for the light and the amount of transfer noted. The dominant stimulus might be the choosing of sound, the avoiding of an electric shock, etc., followed by a substitution of one of the other stimuli. In every case, he says, 'the learning of a response to one situation, having a given element or stimulus as the dominant or controlling factor, is a help in learning the same response stimulus, not present before, as the dominant controlling factor; . . . in some situations the learning process for one sort of dominant stimulus is actually reduced in length so much by first introducing another dominant stimulus, that the time and effort for both is less than for the one alone.' He asks, 'would it be too much of a hazard to hypothesize thus: Variations in stimuli allow of positive or advantageous transfer effects, while variations in response, an aspect of the problem which has not been tested in these experiments, produce negative effects?' "

Brockbank taught his rats a circular maze, then gave them various other problems for thirty days. When the animals were retested in the original maze there was no evidence of retroaction. Wiltbank, in 1919, did some work which was really a continuation of Webb's experiments. Using rats in various mazes, he

found evidence of positive transfer, and little else of any importance. Dashiell, the next year, also reported transfer effects in various types of maze learning.

Hunter, in 1922, studied the interference problem with rats in a T-maze. The stimulus was a light, and in the first habit the rats were trained to turn to the right when the light appeared and to the left when it did not. In the second habit the animals ran left for light and right for darkness. The results showed a marked interference—the first habit being mastered in two hundred and thirty-six trials, while the second required more than six hundred. The interference seemed to occur largely in the first half of the learning process.

In general, as Oakland Maupin says in her excellent summary of the whole subject: "Because of the difference in the methods, apparatus, etc., used by the experimenters who have worked on transfer of training, it is impossible to draw any general conclusions with regard to it. It seems fairly well established that under certain conditions positive transfer can be obtained and under certain other conditions negative transfer is found and that such transfer may be both general and specific in nature according to the conditions of the problem."

The word *memory* is not convenient for use in connection with animal problems, so we employ the term *retention* instead, having reference to the fact that a habitual response often persists after a period of disuse. If the response is as clear-cut and definite at the

end of the rest-period as before, we say that retention is perfect. This is, of course, not usually the case. The only way to be certain that a given habit has not been retained is to learn it over again; if it requires as much time and effort to master it the second time as it did the first, we may say that there is no retention—the habit was completely *forgotten*. There are at present no properly standardized methods of making retention tests, and the data that we have are not very conclusive or satisfactory.

Yerkes and Huggins, in 1903, found that the three craw-fish which learned a simple maze habit retained their ability for more than ten days, and Yerkes reported a green frog which was able to repeat a maze performance after thirty days had passed. Allen, who worked with guinea-pigs in a simple rectangular maze in 1904, reported that his subjects ran nearly as well as ever after a sixty-day rest. Porter's sparrows, which learned the Hampton Court maze in 1906, were unable to give a perfect performance thirty days after the end of the training period. Rouse, in 1906, taught six pigeons a modified Hampton Court maze, and remarked that the habit persisted for "several weeks."

Yerkes, in 1907, found that the dancing mouse which had mastered a black-white discrimination habit retained it perfectly for about two weeks. The results after a four-week interval showed great individual differences. Thorndike, in 1911, carried out a series of experiments with chicks, which gave nearly per-

fect trials in a simple maze after twenty days had elapsed. Hunter's experiments with pigeons in a modified Hampton Court labyrinth showed that the retention is practically perfect for four weeks, when there was no intervening training. Yerkes, in 1912, established a T-maze habit in an earthworm which seventy trials in the opposite response failed to break. Breed, in the same year, reported that his chicks retained the ability to make the blue-black responses correctly after a thirty-day rest.

Turner, 1914, found that his cockroach suffered a lapse of memory after about twelve hours, and made many mistakes in a very simple maze. Sackett's porcupines lost much of their ability to run the Hampton Court maze in about two weeks. Schwartz and Safir, who used fiddler crabs in a triangular labyrinth, found that the habit does not last over two weeks. Churchill, in 1916, put a gold-fish through a simple three-chambered maze, and reported a fair degree of accuracy after thirteen days of idleness. Brockbank, in 1919, found that the Watson maze-habit lasted for at least thirty days in white rats. The work of Randolph, who used goats in a T-shaped maze in 1922, indicates that these animals retain simple habits for at least six months.

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